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EDITORIAL.

CEREBRAL INOCULATION IN DOUBTFUL CASES OF RABIES—literature on this disease quite abundant—its symptomatology positive, the post mortem lesions however, in many instances insufficient for a positive diagnosis—importance of some lesions—result of Pasteur's investigation—cerebral inoculation always gives the same result, the same symptoms and the same lesions—death takes place always in almost a specified time—cerebral inoculation of foreign bodies will give rise to meningitis, but that, properly done with rabid nervous substance, will produce rabies with the specific symptoms, duration, and termination and not to the manifestation of rabiform signs. THE DECLARATION IN CASES OF CONTAGIOUS DISEASES—it is the first and probably most important sanitary measure—it is ignored by many and objected to by the majority—laws and regulations make it obligatory—result of its ignorance. TRACHEAL INJECTIONS—results obtained with them by Dr. G. Levi—counter experiments as performed at the Royal Veterinary School of Milan. HEREDITY OF GLANDERS—experiments and conclusions of Messrs. Cadeac and Malet—the mother seldom transmits it, the father never. DISEASES OF THE HEART—conclusion of the excellent translation, by J.S. Meyer, Sr., of the able paper of F. Blazekovic on the subject.

CEREBRAL INOCULATION IN DOUBTFUL CASES OF RABIES.—Notwithstanding all that has been written on the subject of rabies in the dog, it is quite a doubtful question whether our knowledge concerning this frightful disease has yet become complete. The portion of its history with which we are best acquainted is probably its symptomatology, and though it may assume a variety of aspects, there are some of its appearances which will always justify the veterinarian in pronouncing a positive diagnosis upon an inspection of the living patient. And among these there is one so peculiar and so characteristic, that when once cognizant of it he may safely pronounce his condemnation of the animal without even seeing the case. This may be denominated the *vocal test*,

and consists in the change of voice of the diseased animal, which literally condemns himself out of his own mouth. No one who has ever heard the peculiar *rabid bark*, will fail to recognize the sound when again heard. Independently of this, the general sum of the symptoms, the characteristic changes, the progressive paralysis, and the final termination of all in the closing scene within the brief period of from forty-eight hours to four days, are too generally comprehended, and have become too familiarly known, as they have been observed and described by Fleming, Youatt, Bouley, Galtier and many others, to have room for the slightest shade of doubt in the mind of a professional man called to pronounce upon a case.

If, however, such is the case in the diagnosis of rabies during life, while the paroxysm, the progress and the mode of death of the patient are *specific* and *characteristic*, the same cannot be affirmed in reference to the cadaver of an animal dead or killed under suspicion of laboring under the disease. The condition of the fauces and of the digestive apparatus, the sometimes absent lesions of the nervous centers, the peculiar aspect of the abdominal organs, the presence of foreign substances in the stomach, and the state of the urinary apparatus, which according to some authorities always presents the special feature of an empty bladder, well retracted into the pelvic cavity—all these lesions have in many instances been the only means of reading a diagnosis, and however carefully the inquiry may have been made, there has been ground for doubt as to the positive nature of the natural or artificial cause of death.

In the year 1881, however, during the investigations conducted by Pasteur in relation to hydrophobia, for the determination of its nature, character, method and history, that learned chemist was brought to the conclusion that not only the virulent element of rabies existed in the nervous system, in the brain and in the medulla, but that by the operation of cerebral inoculation of this infected nervous matter, he would in a short time develop rabies under various forms. His communication was important to all who looked at this result, but to the veterinarian, from any point of view, upon whom the decision of the question arising in the

case of the dead animals must often devolve, it was a most valuable assistance, by enabling him to dispense with the long and tedious and not always successful process of ordinary inoculation, and replacing it by one of short duration, and which was *always positive*, not negative, *in its results*. The experiments had been made time and time again. Hundreds, and indeed, thousands of these tests have been made, and the operation is so simple that failure, even through imperfect manipulation, has been followed by death in only an infinitesimal proportion, and to-day the whole world is practicing cerebral inoculation for the development of rabies, either for information, experimentation or inoculation. To say, then, that "the method of demonstrating rabies by direct inoculation of the brain is fallacious," is an assertion that all intelligent and unprejudiced scientists will pronounce erroneous.

This assertion, made before a scientific body of this city, was supposed to be supported and confirmed by the report of a few experiments in which cerebral inoculation of foreign substances gave rise to meningeal disturbances, and to symptoms which *were claimed* to be of a rabid character. Of course it will not be denied that meningitis may sometimes result from cerebral trephining, from improper manipulation and from the introduction and presence in the brain of foreign substances, as exemplified and reported in the July number of the *Journal of Comparative Medicine*, and a careful reading of these reports evidently shows that no other result could have been looked for. But to affirm the fallacy of cerebral inoculation from these premises is to take a great liberty with the facts and the logic of the case, and is taking quite too wide a stride in reach of a conclusion.

Let the experimentators who have come to this rather hasty conclusion repeat their experiments a sufficient number of times, no longer with foreign substance, but with *fresh material* obtained from a rabid or suspected rabid animal; let them closely watch their patients, and look for the *well confirmed symptoms* of the disease; let them *carefully* note the regular method in which the mortality will take place, after an almost positive length of time, *always the same* for those in which the rabid brain has been used; let them observe the animals that may die from meningitis, care-

fully noticing the symptoms, the *duration of the disease*, etc., and if they will conscientiously lay aside their fancy for any *bogus* hydrophobic symptoms which may make their appearance, they will certainly abandon the mistaken conclusions, which, were they to become established and accepted, would deprive investigators in this field of one of the best means of recognizing rabies in the cadaver.

The Declaration in Cases of Contagious Diseases.—In a practical, sanitarian point of view, the first and one of the most essential measures touching the prophylaxy of contagious diseases is the declaration of the existence of the obnoxious affections. Such a declaration, in a report to the proper authorities of the presence of the designated disease, recognized and pronounced as contagious either to man or to animals, or to both, and by which the condition of the disease is authentically established, having been obtained as a basis for effective future proceedings, it will be a comparatively easy task to determine the steps to be subsequently taken to insure the result desired, of effectually checking the spread of the obnoxious malady, the only method of effecting which and of saving the bulk of our domestic animals is the prompt destruction of the diseased individuals. As important and necessary as this measure is in its results, it is naturally and inevitably, of all sanitary regulations, probably the one which is the most difficult to carry into effect. It is opposed and evaded by the owners of the diseased animals, and it is offensive to the sympathies of interested neighbors, fearfully apprehensive, perhaps, of the time when their turn may come to encounter the evil; and moreover, it must be admitted that it is distasteful to even the veterinarian himself, who feels his professional conviction of the right and necessity of the heroic treatment, and his interest as a practitioner drawing in contrary directions—for the practitioner who gives a hopeless diagnosis and a prognosis which is a death sentence, will find it hard work to maintain a pre-eminent popularity, or to avoid giving offence at times to inconsiderate people.

The delicacy and embarrassment unavoidably involved in the issue of the condemnatory declaration seem to have been appre-

ciated and considered by legislators in their action upon the measures involving the discussion of sanitary matters, and as a means of relieving the official executants of the law from the unjust odium which is sure to be incurred by every man who faithfully performs the unpleasant duty committed to him, by making its execution imperative and obligatory by law. For example, on the 11th of September, 1884, a section of the Sanitary Code of the Health Department of the City of New York was passed which read as follows:

"SECTION 185. Every veterinary surgeon who is called to examine or professionally attend any animal within the city of New York having the glanders or farcy, or any contagious disease, shall within twenty-four hours thereafter report in writing to the Board of Health of such city the following facts, viz.: 1st, a statement of the location of such diseased animal; 2d, the name and address of the owner thereof; 3d, the type and character of the disease; and to request compliance with the same."

Similar regulations may doubtless be found in other orders, to be, it is feared, ignored or violated, with whatever results may be expected from ignorance or violation of the law.

A case of quite recent occurrence may be cited, which is fairly illustrative of the matter under discussion.

A veterinarian was called to examine an animal, in which he thought he recognized glanders, but, dissatisfied with his diagnosis, the owner called in two other veterinary surgeons, who derided and denied the first diagnosis, and treated the animal until he died, after a few days illness. The result was an uncertainty as to the diagnosis, placing the first practitioner in a decidedly unpleasant position, and a failure and error as far as sanitary medicine is concerned. All this might have been avoided had the declaration been made to the proper authority, who would have, *officially*, either confirmed or overthrown the diagnosis. The veterinarian who neglects to obey a sanitary order, such as the required declaration, does injustice to his employer, to himself, to his profession and to the community.

TRACHEAL INJECTIONS.—In 1883, Dr. G. Levi, of the University of Pisa, published a little work on the therapeutic advantages to

be obtained by tracheal injection in the treatment of all diseases, and gives several reported cases of recovery from glanders and farcy.

At that time the new mode of administering medicines created some interest in the profession, and carefully written notices and reviews of the book from time to time appeared in veterinary journals. The veterinary periodicals of Italy have since then published a variety of articles on the subject, relating principally to the two diseases mentioned, and the learned director of the veterinary school of Milan, Dr. Lanzillotti Buonsanti, has, in several numbers of the *Clinica Veterinaria*, published numerous communications upon the curability of glanders by tracheal iodurated injections.

The importance of the subject has naturally excited an interest amongst veterinarians, and many experiments have been reported, the majority of them having proved unsuccessful. In fact, if there have been recoveries, they seem to have been obtained exclusively by Dr. Levi, whose principal claim is that the disease is always curable in its first stage, or at a time when the lesions are recent, etc. Dr. Levi also claims in its favor that the new treatment is perfectly inocuous and harmless in its influence.

In the excellent annual report of the Royal Superior School of Veterinary Medicine of Milan, the worthy director, Dr. Lanzilotti Buonsanti, gives an account of a number of experiments conducted in the Milan School, referring to the various points claimed by Dr. Levi, and his conclusions are that: 1st, the condition and serious character of glanders are not in proportion to the clinical symptoms, and that a favorable prognosis of recovery cannot be given from them alone; 2d, that the method of tracheal injections not only does not cure glanders, but that they accelerate the evolution of the symptoms; 3d, that irritating tracheal injections cannot be continued for a length of time without giving rise to a severe traceho-bronchitis and peritracheitis of a serious nature. The reports terminate in saying that it is most probable that all the cases of so-called recovery which had been reported were but those of temporary improvements—"white-washed" cases, so-called—in which a new development of the disease would

take place as soon as good hygienic and therapeutic effects, to which the patients were submitted, would have ceased.

Heredity of Glanders.—The participation of the offspring in the diseases of their parents, and the development in the former of the germs existing in the constitution of the progenitor, are facts which cannot be ignored. But just to what extent and just in what proportion the idiosyncracies of the parents contribute, by transmission of germs or otherwise, to influence or originate the characteristics of the mutual product, remain, with kindred questions, yet to be explored. Contagious diseases are transmissible from the mother to the fœtus by the placenta, in many affections, and there can be no doubt that microbes carried by the blood have more chances to contaminate a fœtus than those which are enclosed in an organ in which they must meet many obstacles to their dispersion through the economy, by the character of the changes to which they give rise.

Amongst the diseases whose maternal heredity is well known, both in man and animals, are small-pox, measles, scarlet fever, syphilis, chicken cholera, symptomatic anthrax, anthrax fever, pleuro-pneumonia, small-pox in sheep, and tuberculosis.

The influence contributed by the male parent in the transmission of contagious diseases is, however, more limited than that of the mother, although syphilis in man and tuberculosis in animals are well known to be inherited by the offspring of parents affected with those diseases. The extent to which these conditions influence the product of glanderous parents is a question of great interest, and which Messrs. Cadeac and Malet, of the Toulouse veterinary schools, have endeavored to solve. By a number of clinical observations, and a number of experiments, they have obtained the following facts:

1st. Out of twenty-nine clinical observations, there is not one which conclusively proves the transmission of glanders from the mother to the fœtus.

2d. Out of twenty-one experiments, this transmission has been proved only four times.

3d. Out of fifteen animals born of healthy mares and glan-

derous stallions, two only have presented doubtful symptoms, and insufficient to positively prove that glanders is transmissible by the genital apparatus of the male.

4th.—Seven experiments made to elucidate this special point have always given negative results, even in cases where both parents were diseased.

5th. The product of glanderous parents, watched during six months, have never presented symptoms or lesions, and the test of inoculation has always been followed by negative results.

6th. The inoculation of virulent liquids is equally as efficacious upon the products of glanderous parents as it is upon subjects of pure origin; consequently they did not have glanders in nature, nor in germ, nor did they enjoy immnuity from it.

The conclusion of the interesting and able investigation made by the Toulouse professors, is that glanders is seldom transmitted from the mother to the fœtus, and never by the father.

Diseases of the Heart.—In our present issue we conclude the series of papers communicated by our associate editor, Dr. J. Myers, Sr., on the Diseases of the Heart of Domestic Animals, by the publication of the portion relating to treatment indicated. Of all the communications we have offered to our readers on this side of the Atlantic, probably there has never been one possessing such an amount of interest and value as this translation from F. Blazekovic. The literature on this subject is so deficient, that to have made a good German version, accessible to all English reading people, is on the part of Dr. Myers a most meritorious task, by which we hope American veterinarians will largely profit. We regret, however, that the articles in question were not prepared for the columns of the Review under an arrangement which would have made feasible their publication in a single volume.

The Pasteur Inoculation in Russia.—The Odessa correspondent of the *Daily News* telegraphs: "From the 24th inst., all persons in Russia bitten by rabid dogs and wolves will be treated in this city by Dr. Gamelei, for whose initiatory operations lymph has been supplied from M. Pasteur's laboratory."

ORIGINAL ARTICLES.

DISEASES OF THE HEART IN DOMESTIC ANIMALS, ESPECIALLY THE HORSE.

By Fr. Blazekovic.

(*Translated by J. C. Meyer, Sr., V.S.*)

Continued from page 105.

Therapeutics of the Diseases of the Heart.

The therapie of diseases of the heart, if founded upon scientific principles, is of importance, provided it is stripped of empiricism. For, by therapeutics it is not meant to interfere nolens volens, or to leave the disease to chance, the healing power of nature, but rather it is absolutely necessary as surgeons, to convince ourselves whether we have discerned the extent of cure, mere improvement, alleviation or total incurability. After careful examination it soon becomes obvious whether we have to do with primary, acute, imflammatory, or with secondary, chronic, organic defects of the heart and their sequelæ.

If the examination establishes acute inflammatory diseases of the heart, then hope of recovery should never be abandoned; but on the other hand we must not take the matter too optimistically, especially in the presence of the owner. But we may be satisfied that science of to-day does not possess a modus operandi by which most organic derangements of the heart might be regulated, still being confined to a palliative and symptomatic healing process, but we can give relief to many an animal and render him serviceable for light farm work.

In the first place, a correct diagnosis of the affection of the heart is the main object, then in relation to consecutive and concomitant ailments it is also of eminent practical value, since the possibility of a cure, the course of the therapeutics, particularly in consecutive diseases, enables us to readily discern the heart affection. A correct dietetic procedure is exceedingly important. Comparisons between animals which have been treated in a dietetic manner, and others which have suffered neglect, or labored under other unfavorable circumstances, impress upod us the great

value of the proper treatment, nursing and attendance of animals suffering with heart disease. Diet is at the head. Many a noble race horse has fallen victim to injudicious feeding; my experience in this direction has been particularly extensive. With moderate, not too proteinaceous food, more hay than oats, turnips, cut straw, etc., and light work, such animals will prove serviceable for a long time; but herbage and pasturage are specially recommended for such patients. Whenever possible it is always advisable to put a horse afflicted with heart disease to a stud or pasture.

I have had mares in the stud for years which were emphysematous to a high degree, with considerable organic defects of the heart which, notwithstanding these great defects, could accomplish parturition without any disadvantage. Nothing acts so beneficially upon chronic affections of the heart as the agreeable breeding life.

Proceeding with the direct therapeutic treatment, we will first of all consider the acute inflammatory diseases of the heart which by timely interposition warrant the most hope of recovery.

In all inflammatory diseases of the heart a thorough antiphlogistic treatment is in order. The successful remedies in the whole antiphlogistic procedure are, above all, venesection at the proper time and cooling agents. The beneficial effect of venesection at the outset of the inflammatory hyperamie, when the impulse of the heart becomes manifest, and the murmur rattling, is not to be doubted. Indeed it sometimes happens that after a few hours, hyperamie in the cavity of the heart and myocardium disappears. Next are the local cold applications. A bag containing cracked ice applied to the region of the heart and thorax, producing a continual trickling of fresh cold water, in this kind of heart disease has an exceedingly good effect. Especially does the cold act in a regulating manner upon the violent action of the heart, and upon the lowering of the temperature of the body. Also in all chronic defects of the heart where a violent alteration of the heart is called forth by an attack, the application of local cold will always compensate the physician and remove such violent intervals. The applications must be made on such parts

only, where chilling need not be feared. A light purgative (salts is preferable) will aid the treatment. Drastic cathartics are to be avoided under all circumstances. Epsom salts, natrium and cream of tartar given in frequent doses are sufficient, and have been proved.

The regulation of the heart's action is highly important. Indeed it is one of the chief conditions for a successful cure. To this end digitalis and natron salycil. are at our command. I cannot recommend natron salycil. too highly, for when digitalin fails, it may still be relied upon. Large doses of 30.00 grains, two to three times daily, with two to three grains quinia, has always produced excellent results, especially in such cases where typhus, anthrax, influenza, pericarditis and myocarditis accompanies constitutional diseases, which very often occur.

The antiseptic and temperature-lowering effect of the salycilic acid as regulator of the heart's action is most effective in the form of natron salycilic. Digitalin needs no commendation.

The proper use of aconite, also opium in excessive frequency of the pulse, and the latter in too violent alteration of the heart, will be indicated in those cases where a discontinuance of digitalin is signified. After administering digitalis for two or three days without success, we may sometimes have the desired result with the above mentioned narcotics.

As soon as the exudative stadium is established, be the exudation of a fibrinous, plastic or serous character, we must in the first place endeavor to check it, then remove it. This is often a difficult task, nevertheless it can be accomplished. Already at the beginning of the disease, the suppression of the exudation must be acted upon, and to this end the already mentioned antiphlogistic treatment is well adapted. Besides these, quinine in all its compounds is to be recommended. It is to be given two or three times per day, in one, two or three grain doses in combination with natr. salycil., digitalis, aconite, opium, just as the condition of the patient demands, and later on with an addition of kali-jodatum. The result is often surprising. Kali-jodat. 3.00 in four litres of well water is the best form of medication.

I here acknowledge being a heretic to some degree, in apply-

ing costly medicaments in the veterinary practice, but I proceed from this point of view, not to give that which is cheapest, but that which is most appropriate. Of course I would not order twenty dollars worth of medicine for a horse which is not worth fifteen dollars. But in cases of valuable race horses, which predominate in my practice, I cannot ask whether the medicine is cheap, but whether it answers the purpose. The owner of such a horse does not inquire as to the cost of the treatment, but if ever possible he wants to save his valuable animal, sometimes worth thousands, even if it be only for breeding purposes. Where circumstances will not admit of giving quinine, a diuretic or purgative may be given a trial, such as cream of tartar, tartar emetic, turpentine, baccæ juniperi, ononis, petrosilei, cantharid., etc., in short, the expedients of the old school. If relapses occur, the antiphlogistic treatment must be repeated. If the diseases of the heart during their course assume a chronic character, appearances of auæmie become manifest quite often, which require the employment of iron preparations, I can especially recommend ferrum lacticum as the most soluble of all iron preparations; ferri. sulph. will also answer the purpose.

Herewith the therapeutical treatment of the inflammatory diseases of the heart are given. These are not recipes and formulas, but the intelligent practitioner will easily understand without further specifications.

The treatment of nervous affections, as palpitation of the heart, neuralgia, cramp, etc., is much more successful. The application of aqua. laurocerasi, twenty per dose, mixed with opium or aconite, in palpitation is generally crowned with success. In nervous palpitations we will find that digitalis is indispensable; still, should it fail, narcotics would be in place. After quieting the nervous alteration it is well to give one or two doses of cream of tartar with natr. nitric.; the disturbances of the circulation which might possibly follow would thereby be paralyzed. Intense nervous alteration, in which dyspnœa is often present, requires the use of natron salycil., venesection and antiphlogistic remedies. A nervous alteration is nearly always present in laminitis in horses, and rheumatism of the joints, against which

natron salycil. is a specific. In most cases it will remove the threatening symptoms of the heart.

The therapie of organic defects of the heart is much more hopeless. As already mentioned, we cannot hope for a total cure, but must restrict ourselves to a palliative and symptomatic interference. In the *Annales de Medicine Vétérinaire*, 1881, No. 9, Prof. Degive gives an account of symptomatic treatment of two cases of defects of the heart with momentary violent action. But only such momentarily-appearing violent action of the heart are brought for treatment which present themselves with or without secondary appearances, as hemorrhage, difficult respiration, etc. According to the degree of alteration we must interfere with strong or weak remedies. In some cases a dose of natron salycil. and digitalis is sufficient; in others bleeding and cooling agents are indicated; especially then, when danger of suffocation, severe emphysema, or even œdema threatens the life of the animal.

Such momentary alterations occur in nearly all kinds of defects of the valves, hypertrophy of the heart, aneurism, expansion, etc., and are the result of improper usage or diet; still they often set in without any traceable cause.

As soon as the violent symptoms are allayed, natron salycil. and digitalis are to be discontinued, otherwise paralysis of the heart is likely to occur. At all events both remedies must be used cautiously in all such chronic heart diseases where a pronounced weakness of the heart and diminished impulse are present, as their action upon the inhibitory nerves of the heart can cause unfavorable results. In such cases we might try bromide of potassa, clysters of chloral and morphia injections; for if paralysis of the heart does not actually set in, still a dangerous weakness of the heart will undoubtedly remain.

If chronic roughness, insufficiencies, including proven or suspected malformations, are to be alleviated or brought to a standstill, a persistent use of absorbing and soluble medicaments in small doses are indicated. In violent impulse of the heart, tartar emetic in small doses is to be recommended, but it must be kept up for some time. Its effect upon the inhibitory nerves of the

heart by means of the sympathetic nerve is, besides its absorbing qualities, quite effective. The continued use of cream of tartar, then of calomel, might also call forth some improvement. For emphysema (in consequence of defects of the heart) horse dealers will administer purgatives, and thereby bring on momentary improvement. Of course they do not know how the result was attained.

It is an established fact that effective purgatives will afford relief in chronic affections of the heart. The occasional administering of the same would be quite rational, but it must not be carried too far, and debilitate the animal too much.

The treatment of the consecutive diseases, together with the defects of the heart, must correspond to the nature of the existing consecutiva.

THE ÆTIOLOGY OF RABIES

AND THE METHOD OF M. PASTEUR FOR ITS PREVENTION.

By Hermann M. Biggs, M. D.,
Instructor in the Carnegie Laboratory.

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In the introduction to his last communication upon rabies, referring to this former method, he says: "The prevention of rabies, as I have described it in previous notes in my own name and the names of my collaborators, certainly constitutes a real advance in the study of this malady—an advance, however, which is more scientific than practical. Its application was dangerous. Among twenty dogs that had been thus treated, I could not affirm that more than fifteen or sixteen had been rendered refractory to rabies. It was expedient also to complete the treatment by the inoculation with an exceedingly virulent virus for control, in order to confirm and strengthen the refractory condition. Finally, prudence dictates that the dogs should be kept under observation for a longer time than the period of incubation of the disease produced by the direct inoculation with the last virus, and thus an interval of not less than three or four months was required to afford perfect assurance of a fully refractory condition. It was therefore neces-

sary to obtain, if possible, a more rapid method, and one giving absolute security to dogs. After almost innumerable experiments, I obtained a preventive method, practical and prompt, with which sufficiently numerous and assured successes have already been obtained upon dogs to give me confidence in its general applicability to all animals and to man himself."

This method, as gathered from Pasteur's communication and from his assistants while in the laboratory, rests especially upon the following observations:

In the human being or in animals dead of rabies, the rabic virus is present in great abundance and in a pure form in the central nervous system, and especially in the medulla oblongata and the spinal cord. This observation is not wholly original, but is in confirmation of the experiments made by Rossi and Hertwig, who succeeded in producing rabies in healthy dogs by inoculations with portions of nerves obtained from animals dead of rabies. Pasteur found that animals inoculated with portions of the medulla or spinal cord, and then brought into suspension in a sterilized bouillon, developed the disease with greater certainty than when inoculated with the saliva of rabid dogs. This is especially true if the inoculations are made upon the surface of the brain underneath the dura mater. Nearly all species of animals are susceptible to the disease; but the virus undergoes certain modifications as regards its virulence and its period of incubation after passage through different species. In the transmission of the virus through a series of monkeys, the virulence gradually becomes diminished and the period of incubation longer. If, on the other hand, the virus is passed through a series of rabbits, the virulence is gradually increased and the period of incubation becomes shorter. The average period of incubation of rabies in rabbits which have been inoculated under the dura mater, after trephining with the spinal cord of a dog just dead of the disease, is fifteen days. However, if the virus is passed successively through a series of rabbits from the first to the second, from the second to the third, and so on, always using for the inoculation of the following animal the spinal cord of the one just dead of the disease, and in each case making the inoculations underneath the dura mater in the same manner

as in the first instance, the period of incubation becomes gradually shorter, and, after the passage through a series of twenty-five rabbits, the time is finally reduced to eight days. In the transmission of this virus through a second series of twenty-five rabbits, the period of incubation is further reduced to seven days, but at this point apparently becomes stationary, as this time is not materially diminished in the passage through a third series of forty animals. Commencing these experiments in November, 1882, at the end of three years Pasteur had inoculated in this manner successfully a series of more than ninety rabbits, without having had this series once interrupted or broken. He says of this: "Nothing is easier, therefore, than to have at one's disposal, after considerable intervals of time, a virus of perfect purity always identical, or nearly so. This constitutes the practical point of the method."

If, now, the cord is removed (with all possible care to prevent contamination with foreign matters) from a rabbit just dead from rabies, and is suspended in a jar the air of which is artificially dried and kept at a constant temperature of 20°C., it gradually loses its virulence until, at the end of about fifteen days, this has become quite extinct. The time required for the extinction of the virulence is somewhat dependent upon the thickness of the spinal marrow and upon the external temperature. The spinal cords which have been subjected to this process of desiccation for a shorter period than fifteen days possess a gradually increasing virulence inversely proportionate to this time. The conditions remaining the same under which the cords have been preserved, the degree of virulence is always constant, or nearly so. If the infectious cords, while in a moist condition, are protected from the atmospheric air and submerged in carbonic-acid gas, the virulence may be preserved unaltered for several months, providing only that no foreign micro-organisms gain access to them. According to Pasteur, "these observations constitute the scientific points of the method."

To render a dog refractory to rabies the following method is pursued:

On the first day the animal is inoculated with a portion of the cord (removed from a rabbit dead of rabies appearing after

an incubation period of seven days) which has been preserved in a dry air for about fourteen or fifteen days, and has thus been deprived of all its virulence. On the following day a cord twelve or thirteen days old is used, and on the succeeding days the inoculations resolve themselves into about the following order:

3d	day,	spinal	cord	11	days old.
4th	"	"	"	9	" "
5th	"	"	"	7	" "
6th	"	"	"	6	" "
7th	"	"	"	5	" "
8th	"	"	"	4	" "
9th	"	"	"	3	" "
10th	"	"	"	2	" "
11th	"	"	"	1	day "

This is about the order followed, more inoculations being here described than are perhaps essential or usually employed in practice. The inoculations with the cords which have lost their virulence through the process of desiccation gradually render the animal insusceptible to the most virulent forms of the virus, so that, finally, no symptoms follow inoculations with virus which produce rabies in dogs with great certainty in from eight to ten days, when the animals have not been previously protected by inoculation with the less virulent cords. In this manner dogs may be rendered refractory to rabies, even when they are subsequently inoculated, underneath the dura mater after trephining, with the most virulent virus.

The efficacy of this method was proved by subjecting fifty dogs to these inoculations, with success in every case in rendering the animals refractory to rabies. After having arrived at these results, Pasteur felt justified, after consultation with Dr. Vulpian and Dr. Grancher, in subjecting to the treatment a boy, Joseph Meister by name, who had been bitten by a rabid dog and who unexpectedly presented himself at the laboratory for treatment on July 6th.

This boy had been bitten on July 4th in fourteen places by a dog which was supposed to be undoubtedly rabid. Many of the wounds were very deep, and a portion of them only had been

cauterized twelve hours after the accident, so that, in the opinion of Dr. Vulpian and Dr. Grancher, the boy seemed certainly destined to develop hydrophobia. He was accordingly subjected to the following inoculations:

July 6th,	8 P.M.,	spinal marrow of	June	21st,	15	days	old.
" 7th,	9 A.M.,	"	"	" 23d,	14	"	"
" 7th,	6 P.M.,	"	"	" 25th,	12	"	"
" 8th,	9 A.M.,	"	"	" 27th,	11	"	"
" 8th,	6 P.M.,	"	"	" 29th,	9	"	"
" 9th,	11 A.M.,	"	"	July 1st,	8	"	"
" 10th,	11 A.M.,	"	"	" 3d,	7	"	"
" 11th,	11 A.M.,	"	"	" 5th,	6	"	"
" 12th,	1 P.M.,	"	"	" 7th,	5	"	"
" 13th,	11 A.M.,	"	"	" 9th,	4	"	"
" 14th,	11 A.M.,	"	"	" 11th,	3	"	"
" 15th,	11 A.M.,	"	"	" 13th,	2	"	"
" 16th,	11 A.M.,	"	"	" 15th,	1	day	"

Pasteur says of this case: "I dare say that a smaller number of inoculations would have been sufficient, but it is easy to understand that the first trial was made with the observation of every possible precaution."

In order to determine the virulence of the virus used, two rabbits were inoculated each day in the usual manner with the same virus as that used for the boy. These inoculations showed that the cords used on the 6th, 7th, 8th, 9th and 10th of July were not virulent, since they did not produce rabies in the rabbits inoculated on these days. The cords used on July 11th, 12th, 14th, 15th and 16th were all virulent, and were of a constantly increasing virulence. Rabies appeared after seven days of incubation in the rabbits inoculated with the cords of July 15th and 16th, after eight days in those of the 12th and 14th, and after fifteen days in those of July 11th. Notwithstanding the fact that, in the last inoculations, virus was used which was far more virulent than that of rabid dogs, still these inoculations were followed by no symptoms, and now that more than six months have elapsed since this boy was bitten, he yet remains perfectly well.

The methods used for the preservation of the spinal cords and

for the preparation of the virus for the inoculations are exceedingly simple. After the death of an animal from rabies, the spinal cord is removed with the greatest care and suspended in a flask about half a metre high. These flasks have rather large mouths, which are tightly plugged with sterilized cotton and the bottoms are covered about one inch deep with caustic potash to absorb all moisture from the air. The room in which the flasks are placed and in which the virus is prepared for the inoculations is kept constantly at a temperature of 20C°. The lower the temperature, the longer the virulence of the cords may be preserved.

The virus used for the inoculations is always prepared fresh, and in doing this a piece, a few millimetres in length, is first snipped off from a cord that has the desired degree of virulence, as determined by the length of time that it has been suspended in the flasks. This piece is then rubbed up with a small quantity of sterilized chicken bouillon in a small conical glass. *There is no exact proportion between the amounts of spinal marrow and bouillon* used in preparation of the virus. The spinal marrow, after being in this manner brought partially into suspension in the bouillon, soon settles to the bottom of the glass, and the comparatively clear fluid above is used for the inoculations. These are made in the case of the human being by hypodermic injections into the cellular tissue of the anterior abdominal wall. An ordinary Pravaz syringe is used for this purpose, and from half a syringeful to a syringeful of the bouillon is injected, in each case depending upon the age of the person under treatment. The spinal marrows that are used in these inoculations are always those obtained from rabbits which have died from rabies after a period of incubation of seven days.

These inoculations are not followed, either immediately or remotely, by any constitutional disturbance, so far as I was able to determine, and they produce only a slight local tenderness—scarcely more than that caused by a hypodermic injection of morphine.

The order of the inoculation, as followed now in the treatment of the human subject, is practically the same as that described above for rendering dogs refractory to the disease.

Since Pasteur's communication to the French Academy, no October 26, 1885, his laboratory has been besieged by many persons from all countries who had been bitten by rabid dogs, and, after the lapse of only a little more than two months, up to January 1, 1886, nearly one hundred and forty persons of all ages and nationalities had been subjected to these inoculations. The period that had elapsed after the accident, before the treatment was commenced, varied in different cases from a few hours to thirty-six days. In these one hundred and forty cases, up to the time of my visit to Paris, only one death had occurred, and in this case the child was not inoculated for the first time until thirty-six days after she had been bitten. Shortly after the inoculations were completed this patient died with clearly defined symptoms of hydrophobia. In explanation of the failure to prevent the appearance of the disease in this instance, Pasteur asserted that the period of incubation of the disease as transmitted from the rabid dog had elapsed before immunity had been conferred by the inoculations. This explanation certainly seems to be reasonable when we remember that the period of incubation of rabies in the human being, although usually more than thirty-six days, still not infrequently is less than this time. An interesting and serious question came up in connection with this death. It was, of course, possible that the disease might have been the result of the bite of the rabid dog, or might have been caused by the far more virulent virus use in the inoculations. A solution of this question was obtained in the following manner: The virus of a rabid dog, when transferred to a rabbit by inoculation underneath the dura mater after trephining, produces rabies on an average in fifteen days, while the most virulent cords used in the inoculations produced rabies in rabbits with the greatest uniformity in seven days, and in dogs in from eight to ten days. Pasteur then asserted that if rabbits were inoculated in the usual manner with material from the brain or spinal cord of this child, that if the disease of the child was produced by the inoculation, the rabbits would die of rabies which declared itself after a period of incubation of seven days; but if the disease in the child was due to the inoculation made by the bite of the rabid dog, then disease

in the animals would not make its appearance until about the fourteenth or fifteenth day. Accordingly, rabbits were inoculated from the child's brain removed at the autopsy, and they died with the usual symptoms of rabies, which appeared on about the fifteenth day. The result of this experiment seems very satisfactory, but there is apparently a source of error in the possibility of the periods of incubation of the respective viruses being altered by their passage through the human being.

An important question immediately arises in connection with the conclusions to be drawn from the inoculation of these different persons, and that is in respect to the existence of genuine rabies in the many dogs by which they were bitten. As much care as possible is taken to ascertain this fact, but, of course, in some individual cases it cannot be satisfactorily determined, inasmuch as the dogs are so often killed before the presence of rabies has been absolutely shown or before the animals have been examined by competent veterinarians. In many of the cases there can be no question as to the genuine nature of the malady with which the dogs were affected, and, as the number of persons already inoculated is very large and is being constantly augmented, the importance to be attached to the doubtful cases is proportionately diminished. The conditions obtaining in the cases of the first Americans—four in number, who were sent to Paris for treatment in the care of Dr. F. S. Billings—as regards this point would have been exceptionally satisfactory providing only the dog by which these children were bitten was shown to be rabid. This, apparently, was not the case. Six children and a number of dogs were bitten by the same dog, supposed to be rabid. The animal was immediately killed, but a number of the dogs bitten were placed under confinement. The wounds of all the children were cauterized after the lapse of a few hours. Four of the children were sent to Paris and were treated by M. Pasteur; the two others remained at home.

In this case the dogs which were bitten and placed under confinement, and the two children which remained at home, served as a control on those inoculated, but, inasmuch as more than ninety days have elapsed since the accident and neither the unin-

oculated children nor the dogs bitten have developed any symptoms of rabies, the probable result is evident.

At the time of the writer's visit to Paris there were from twenty to twenty-five persons being inoculated daily, and scarcely a day passed that some new patients did not present themselves for treatment.

The principles underlying Pasteur's method for the preventive treatment for hydrophobia may be stated in a few words as follows: The period of incubation of rabies as transmitted from the dog to other animals and man is very variable, but in man, as a rule, is exceedingly long, being rarely less than thirty days. By the passage of rabies from dogs through a series of rabbits, the period of incubation of the disease may be finally reduced to about seven days, as it occurs in these animals. The virulence of the spinal cords from rabbits dead of a seven-day rabies may be diminished in a constant degree by suspension in an absolutely dry air at a given temperature. Man or animals that have been subjected to a series of inoculations with these cords of diminished virulence, beginning with a very weak virus and gradually going on to a stronger and finally to the most virulent form, gradually become insusceptible to the most virulent virus. The period of incubation of this virus is very short, and the time required to confer insusceptibility to the disease is much less than the ordinary period of incubation of hydrophobia as produced in man by the bite of a rabid animal; consequently, if too long a time has not elapsed after the bite of a rabid dog before treatment is commenced, the person or animal may be rendered insusceptible to the disease before this period of incubation has elapsed. The time that may elapse in the case of the human being bitten by a rabid animal before successful treatment may be commenced is probably, as a rule, not less than twenty days, and in some cases may be considerably more than this, but is perhaps as variable as the period of incubation of the disease in different cases in the human subject resulting from the bites of rabid dogs, and is dependent upon the same idiosyncrasies of the individuals.

As to the interpretation of the prophylaxis against rabies, Pasteur says that the attenuation in the virulence of the rabic

cords by the process of desiccation is explained on the supposition that the continuous contact of the dry air produces a gradual diminution in the intensity of the virulence of the cords until it finally becomes extinct; that the prophylactic method in its application depends for its efficacy upon the employment at first of a virus without appreciable activity, followed by a weak virus, and then by a more and more virulent form; that the diminution of the virulence of the cords is due to an impoverishment in quantity of the virus contained in them, and not to an impoverishment in virulence; consequently in the inoculations the virus used is always identical as regards its virulence, and is variable only in respect to the quantity employed, so that the refractory condition to rabies follows from the employment of very small but constantly increasing quantities of a virus possessing always the same degree of virulence. This interpretation of the method of action of the virus is the more interesting as a new and quite different principle is involved from that obtaining in the vaccine for small-pox or the vaccines devised by Pasteur for the prevention of anthrax, chicken-cholera, and typhus in pigs. In small-pox we have a virus modified in character and virulence by its passage through another species of animal; in anthrax, chicken-cholera, and typhus in pigs, we have a virus modified in respect to its virulence by the conditions of temperature to which it has been subjected during its growth; and, finally, in rabies we have a prophylactic method dependent upon the employment of a virus always constant as regards its virulence, but used in very small and constantly increasing quantities. Apparently, then, already there have been three methods discovered for the preparation of preventive vaccines for different forms of the contagious diseases. The theoretical principle has been also established by the researches of Pasteur, and more recently by Burdon Sanderson, the report of whose investigations has not yet been published, of the possibility of depriving cultivations of pathogenic micro-organisms of their virulence in a constant and persistent degree without in any other respect affecting their morphological or biological characteristics. These methods for preventive vaccination —taken in connection with this principle, which is involved in

one of them—present almost unlimited possibilities in preventive medicine, and possess a significance the importance and far-reaching nature of which it is scarcely possible for us to comprehend.

Pasteur gives in addition an explanation as to the method of action of the virus in the living organism. He says: "It is possible to give a still further interpretation—an interpretation surely very strange at first thought, but which deserves the greatest consideration, especially as it is in harmony with certain results obtained in the observations of phenomena in the life of some other forms of the lower organisms, and especially of several of the pathogenic bacteria which give birth in their cultivations to a material which has the property of preventing their own further development."

After referring to the micro-organisms of chicken-cholera, typhus in pigs, and the cultivation of *Aspergillus niger*, he continues; "Is it not possible that the rabid virus may be formed of two distinct substances, one of which is living and capable of multiplication in the nervous system, and the other is non-living, having the power, when present in sufficient proportion, of arresting the development of the first? I have considered experimentally in a previous communication, with all the attention that it deserves, this third interpretation of the method of prophylaxis of rabies."

I cannot conclude this portion of this paper without a reference to two interesting and important questions that arise in connection with this method of prophylaxis. The first of these is the possibility of transmission of the disease, by animals or the human being subjected to these inoculations, to other animals or human beings during the process of inoculation, or in any period after this time. Pasteur states that he has made no experiments or observations upon this point. The second question is as to the duration of the refractory condition to rabies after the series of inoculations has been completed. This period Pasteur believes to be not less than one year, and to be probably considerably longer than this, but no careful data upon this point are at present at hand.

What conclusions, now, are to be drawn as to the accuracy

of Pasteur's observations upon rabies and the efficacy of his method for the prevention of the disease in animals and man after bites by rabid dogs? Let us review rapidly the experiments that have already been made, and discuss briefly the assertions that are based upon them. Pasteur's first communication upon rabies was in 1880—more than five years ago. At that time he thought he had discovered a new micro-organism in the saliva of a child who had died of hydrophobia, and described a new disease as being produced in rabbits by inoculation with this germ. These observations were a little later shown to be incorrect, and were acknowledged to be so by Pasteur. Since that time he has been studying this disease constantly, and from time to time has reported more or less fully the results of his observations to the French Academy. His investigations have led out in many directions, and the conclusions reached in these experiments, as reported in his last communication to the Academy, have been the result of a gradual development extending over nearly six years of tireless investigation.

As regards the first person inoculated, of whom we have heard so much, it has seemed to me that the evidence is far from satisfactory upon which rests the conclusion as to the rabid character of the malady affecting the dog which bit the now famous Joseph Miester. The indications point rather in quite another direction. It is certainly very unlike the ordinary behavior of rabid animals to fall upon a person with such ferocity and persistency as must have been the case in this instance, when the unfortunate victim was bitten in no fewer than fourteen different places. Apparently the only evidence upon which rests the supposition that the dog was mad is the result of the autopsy, and that certainly does not constitute sufficiently satisfactory proof. But, however this may be, certainly far too much importance, in my opinion, has been attached to this case, and it seems to me that the strongest evidence of the efficacy of Pasteur's method for the prevention of rabies rests, not upon any results thus far obtained in the inoculation of human beings, but upon the results of his experiments upon dogs. If in a single series of experiments he has not only been able to render fifty dogs refractory

to rabies without a single failure, but also has succeeded in preventing the development of the disease in a large number of dogs after they had been bitten, both of which he asserts positively he has succeeded in doing, then the question as to prevention of the disease in the human being is only as to the method of application. For certainly dogs are far more susceptible and liable to the malady than human beings, the character of the disease is evidently the same in both man and dogs, and, if dogs can be brought into a refractory state to the disease, it is not assuming too much to conclude that the same is true in the human being.

The preliminary report of the commission appointed by the Minister of Public Instruction in 1884 to examine and report upon the results obtained up to that time is also of interest here. As incorporated in Tyndall's introduction to the life of Pasteur, it may be summed up as follows:

"Of six dogs unprotected by vaccination, three succumbed to the bites of a dog in a furious state of rabies.

"Of eight unvaccinated dogs, six succumbed to the intra-venous inoculation of rabic matter.

"Of five unvaccinated dogs, all succumbed to inoculation upon the surface of the brain after trepanning.

"Finally, of three-and-twenty vaccinated dogs, not one was attacked with the disease subsequent to inoculation with the most potent virus."

Surely these results upon dogs are of a nature to at least attract the careful consideration and attention of scientific men throughout the world.

But, throwing aside the evidence which we have now at hand as to the efficacy of Pasteur's method—and I think we must admit that it is not altogether satisfactory in character—let us consider the probability of the correctness of these last observations from the light thrown upon the subject by his former work. Does it seem probable that the man who, in the earliest infancy of bacteriology, disclosed the nature and cause of fermentation, and established our knowledge of it on a firm foundation in the face of much opposition and skepticism—the man who disproved the theory of spontaneous generation, and laid the foundation

upon which rests our whole system of antiseptic surgery, who disclosed the nature of the plague in the silk-worms in France and devised the methods for its prevention, who discovered the principles underlying the method of protective vaccination in the disease of animals and who has prepared the only practicable vaccines, who has contributed as much as any man who has ever lived to our knowledge of micro-organisms and their relation to disease—does it seem probable that such a man has given nearly six years of continuous study to the consideration of rabies without achieving some substantial results? Or does it seem probable that he has been dealing all of these years with some form of septicæmia, as has been suggested, laboring under the delusion meanwhile that it was rabies? This I cannot believe, and, if it is not true, then Pasteur's conclusions must be in the main correct, for certainly no one will question his honesty.

Every great theory in science has been met by skepticism, opposition and ridicule. The theory of gravitation, the theory of evolution, the theory of undulation, the atomic theory of matter, the dynamic theory of heat—have all been compelled to fight their way to victory. So it has been with the germ theory of the infectious and contagious diseases. Many members of the medical profession, in spite of the indisputable facts that have been adduced to prove this, still refuse to accept it.

Pasteur's prophylactic method for rabies rests purely on empirical grounds, and can only be fairly judged by the practical results obtained by its use. So far as we know at present, these have sustained the profession of the learned discoverer, and, until they are refuted by further observations, I believe it is unjust to characterize this work of Pasteur's, as has recently been done, as being founded "on untrustworthy experiments and unsound reasoning," deserving "to be rejected and condemned in the interests of humanity as well as science." On the other hand, if future observations confirm the results that now seem to be at least probable, this discovery, added to his other achievements, will rank Pasteur as one of the greatest benefactors of his race that this generation has produced, and, from a scientific standpoint, it will be considered one of the grandest triumphs of the century

VENEREAL DISEASES IN THE LOWER ANIMALS.

Read by Professor WALLEY before the Scottish Metropolitan Veterinary Medical Society.

Continued from page 175.

In the early part of the present year I was asked to examine a retriever dog in whom an obstinate eruption was presented on the skin covering the chin and around the eyes. I was informed that the animal had been under the care of another practitioner for some time, but that the treatment adopted had been unsuccessful. As my attention was only directed to this eruption, I did not think of looking further for the probable cause, though I was particularly struck by the peculiar character of the sores, and unhesitatingly gave the opinion that they were of a specific nature. By the application of naphthol ointment, an agent, I may observe, of great value in some forms of skin disease, and the administration of mercury, the sores rapidly improved and in fact were nearly healed, but several weeks subsequently the owner of the dog, noticing an eruption on the skin of the abdomen, again brought the dog to me for examination. One glance at this eruption satisfied me as to its resource and explained the origin of the sores around the eyes and on the chin; and, on extruding the penis, my suspicions were confirmed, the organ presenting on its external surface several well-marked venereal sores. These sores were treated, successively, with all the known topical remedies, not even excluding iodoform; and simultaneously, constitutional treatment was employed, but all to no purpose, and in the end the penis presented a most loathsome and horrible appearance. With the consent of the owner castration was performed. The good effects of the operation were as satisfactory as in the first case.

I may remark that I have been informed, within the last few days, by one of my pupils, Mr. Carruthers, of Wigton, Cumberland, that during the past summer an equally successful result was obtained by the adoption of this method of treatment in the case of a collie which had been the subject of venereal sores for a considerable period.

Gentlemen,—I have often felt that this treatment, heroic though it be, might be had recourse to with benefit in the human subject. I know that sentiment would oppose itself to the suggestion, but I would ask is it not better that a man who has been unfortunate enough to contract this loathsome disease should be deprived of the means of infecting others, and be himself restored to at least a moderate degree of capacity for enjoyment of the pleasures of life than that he should be allowed to drag out a miserable existence and remain a probable means of disseminating his infirmity ?

The Abortive Treatment of Actinomycosis.

In recent issues of our professional journals, I observe that Mr. J. B. Gresswell has drawn attention to the treatment of actinomycosis by the application of phenol and iodine (iodized phenol.)

I may be pardoned if I suggest that he directs attention to this combination of disinfectants in such a manner as to lead to the supposition that it is new to the veterinary profession.

In a paper read by me about two years ago at Belfast, I directed attention to the great value of a combination of iodine and phenol as a local application in infective forms of inflammation, such as the inoculative leisons in pleuro-pneumonia and in gregarinosis, and it was from observing the beneficial effects following its use under these circumstances, that I was led to advise its employment (to my pupils and to practitioners) in the abortive treatment of actinomycosis—a disease, I need scarcely remark, essentially local in its origin and capable of being arrested in its progress by purely local means. Amongst those to whom I recommended its use was Mr. L. Leach, who at the meeting of the Lincolnshire Veterinary Medical Association, held at Grantham in the early part of the year, informed me that he had successfully carried out this method of treatment.

During the past summer, while on a visit at the house of my old preceptor, Mr. Kettle, of Market Drayton, I advised his assistant, Mr. Rickell, to carry out the treatment in a well-marked case in a cow. Mr. Rickell advises me that the treatment was quite successful.

The *modus operandi* of the treatment by means of which the disease is arrested is, I think, simple; both iodine and phenol are powerful fungicides and active irritants—as the former, they destroy the fungus to which the disease owes its origin; as the latter, they induce a plastic and consequently a localizing or circumscribing inflammation, which has the effect of arresting the extension of the fungus and the lesions to which it gives rise. The surgical operations recommended merely facilitate the application of the potential agents to which I have alluded.

In order to ensure the proper application of remedies, I have had made, by Messrs. Krohne and Sesemann, the following instruments:

(1) A *tongue scraper*, with which is combined (2) a *scarifier*—regulated as to depth by movable screws; (3) an *irrigator*, in the form of a pipette; and (4) a *pair of tongue forceps*, by the aid of which the tongue can be effectually secured (a difficult matter in cattle) and its further manipulation rendered easy. The *scarifier*, I may observe, can be utilized for other purposes than the one for which it was designed, *e.g.*, for scarifying any œdematous swellings on the gums in lampas; while the *irrigator* may be used for similar purposes in the treatment of tumors or for exploratory aspirations.

Intestinal Parasite of Swan.

During the summer I was asked to make a post-mortem examination of the body of a swan with the object of discovering the cause of its death, which had been sudden.

On laying open the small intestines I observed a number of yellowish-colored bodies intimately attached to the mucuos membrane and having the appearance of little masses of yellow paint, but on the application of force I discovered that they were of an organic nature and attached by a pedicle to the membrane. One of my pupils, Mr. N. J. Doyle, who was standing near, suggested that they were parasites, and on removal from the intestine their true nature was readily discovered, and it was further confirmed on microscopical examination by Mr. Gray. From the characters presented I came to the conclusion that the entozoon was a distome, but through the kindness of Dr. Aitken, I was enabled to

submit some of the specimen to Dr. W. E. Hoyle for identification, and by that gentlemen they were recognized as a species of *Echinorhynchus,* probably the *E. polymorphus.* Dr. Hoyle further informs me that they are found in the intestine of the duck and other water-birds, and that in their larval stage they inhabit small freshwater crustacea. They cling, he says, to the mucous membrane by the aid of a number of re-curved hooks or teeth situated on the proboscis.

In our domestic animals we are only, so far as I know, acquainted with one other species of the Acanthocephala or thorn-headed worms (to which this specimen belongs), and that is the *E. gigas* of the pig.

Whether in the swan these entozoa are the cause of much mischief or not, may, I think, be left an open question as, although they adhere firmly to the mucous membrane, they do not appear to induce any marked inflammatory action, nor did I observe any tendency to perforation of the bowel in the manner so often seen in the case of the analogous worm in the intestines of the pig. I also introduce to your notice specimens of a strongylus found by Mr. Percival Snaith (one of my pupils) in the stomach of a rabbit. The nematode has been recognized by Dr. Hoyle as the *S. strigosus* of Dugardin.

PLEURO-PNEUMONIA LEGISLATION.—It is announced from Washington that the Agricultural Committees of the two Houses have finally reached a definite conclusion on the subject of an appropriation for the stamping out of pleuro-pneumonia and other contagious diseases. They have decided to appropriate $100,000 for the use of the Commissioner of Agriculture in carrying out the objects for which the Bureau of Animal Industry was created. He is not restricted in the number of agents that he may employ, but may engage the services of "as many persons as he may deem necessary," and is authorized to use any part of the sum appropriated which "he may deem necessary or expedient, and in such manner as he may think best, to prevent the spread of pleuro-pneumonia." All restrictions are repealed by the compromise agreed upon, and all responsibility for the execution of the

law is thrown upon the Commissioner. He is to act on his own will and judgment. The power to purchase and kill diseased animals is practically unlimited. Both committees having agreed upon the compromise, there is little doubt that both Houses will ratify it, and the President will sign it. The bill has met with considerable opposition, much of this due, doubtless, to the manner in which the work of the bureau has been conducted in the past and the character of some of the men connected with it. A radical change in this respect is needed to secure the hearty support which public measures of this kind should have.—*National Live Stock Journal.*

WHY PASTEUR'S VACCINE FAILS TO PREVENT HOG CHOLERA.*

By D. E. Salmon.

(*Continued from page* 183.)

This communication will be devoted to a further consideration of the difference between the leading characteristics of the French disease called *rouget*, for which Pasteur's vaccine is prepared, and the American hog cholera.

3d. *Results of feeding with organs of diseased animals.*—Feeding the organs of hogs which have died of hog cholera to healthy swine has a most marked and fatal effect, if the healthy animals have not previously acquired immunity by exposure to the contagion. As this fact is contested by some gentlemen who have given the subject "much thought and attention," but whose facilities for observation appear to have been very limited, I give below a list of animals fed with contagious material, mostly organs of dead hogs, from November 18 to March 22, in our experiments at Washington. I would particularly commend this table to the kind and careful consideration of Mr. D. L. Thomas, who claims the distinction of being "the first to take a public stand that healthy hogs will not take swine plague from dead hogs." The animals fed were selected in most cases from herds that had

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not been exposed to the disease for several years, and the material fed was from hogs which had certainly died of swine plague—two points which it is very important to secure if positive results are expected from such experiments:

TABLE SHOWING RESULTS OF FEEDING SWINE-PLAGUE MATERIAL TO HEALTHY HOGS.

No. of the animal.	*Date of feeding.*	*Material Fed.*	*Died of Swine Plague.*	*Days between feeding and death.*
96	Nov. 18	Organs of dead hogs	Nov. 24	6
97	Nov. 18	Organs of dead hogs	Nov. 28	10
98	Nov. 28	Organs of dead hogs	Dec. 5	7
99	Nov. 28	Organs of dead hogs	Dec. 7	9
120	Dec. 5	Germs f'm dead hogs	Dec. 13	8
145	Dec. 5	Germs f'm dead hogs	Dec. 12	7
107	Dec. 9	Organs of dead hogs	Dec. 18	9
108	Dec. 9	Organs of dead hogs	Dec. 21	12
165	Jan. 8	Organs of dead hogs	Jan. 26	18
76	Jan. 28	Organs of dead hogs	Resisted	..
138	Jan. 28	Organs of dead hogs	Resisted	..
139	Jan. 28	Organs of dead hogs	Resisted	..
159	Jan. 28	Organs of dead hogs	Feb. 6	9
156	Feb. 18	Organs of dead hogs	Feb. 25	7
188	Feb. 25	Organs of dead hogs	March 12	15
149	March 5	Organs of dead hogs	March 24	19
158	March 5	Organs of dead hogs	March 21½	16
168	March 5	Organs of dead hogs	March 28	23
175	March 5	Organs of dead hogs	March 22	17
189	March 5	Organs of dead hogs	March 19	14
151	March 13	Organs of dead hogs	March 26	13
152	March 13	Organs of dead hogs	April 3	21
167	March 13	Organs of dead hogs	April 1	19
161	March 13	Organs of dead hogs	April 14	32
170	March 13	Organs of dead hogs	April 4	22
176	March 13	Organs of dead hogs	March 29	16
190	March 13	Organs of dead hogs	March 25	12
115	March 19	Organs of dead hogs	April 8	20
117	March 19	Organs of dead hogs	April 9	21
162	March 19	Organs of dead hogs	March 29	10
169	March 19	Organs of dead hogs	April 10	22
173	March 19	Organs of dead hogs	April 5	17
178	March 19	Organs of dead hogs	April 6	18
172	March 19	Organs of dead hogs	April 6	18
192	March 19	Organs of dead hogs	March 31	12
193	March 19	Organs of dead hogs	March 29	10
174	March 22	Organs of dead hogs	April 16	25

Total number fed, either with organs of dead hogs or with germs cultivated from such organs, 37; number of deaths result-

ing from this feeding, 34; proportion of animals which contracted swine plague and died as the result of taking contagion from dead hogs into their system, 92 per cent. The three animals which resisted the contagion in the above experiments had previously been exposed to the disease; and it consequently follows that every one of the thirty-four pigs which had not been exposed before feeding contracted the disease in so severe a form as to produce fatal results.

In the experiments of Lydtin with the Pasteur vaccine, the vaccinated animals were fed with the organs of hogs which had died of *rouget* to test the immunity which they had acquired. At the same time check animals, which had not been vaccinated, were fed with the same virus to bring out its effects on susceptible animals by contrast with its effects upon those which it was supposed were made insusceptible by the vaccination. The effects of feeding these check animals may, consequently, be compared with our feeding experiments related above, most of which were also with check animals in similar experiments. I have collected these results in the following table:

TABLE SHOWING RESULTS OF FEEDING ROUGET MATERIAL TO HEALTHY HOGS.

Place of experiment.	*No. of animals fed.*	*No. which died.*	*Days between feeding and death.*	*No. sick which recovered.*	*Remarks.*
Heidelberg	4	0	.....	0	Fed twice without result.
Langenzell	4	0	.....	0	Fed twice.
Zuzenhausen	2	1	6	1	One feeding.
Neckarbishofsheim	3	0	.....	0	Fed three times.
Treschklingen	3	1	3	0	One feeding.
Lohrbach	4	0	.....	1	Three fed twice.
Stein	4	1	4	2	One feeding.
Dammhof	3	1	4	0	One feeding.
Rostatt	3	1	4	0	One feeding.
Aspichof	2	0	.....	0	Fed twice.
Geisingen	4	2	3 and 4	0	One feeding.
Messkirch	3	1	4	0	Two fed twice.
Neidingen	2	2	4	0	One feeding.
Pforzheim	4	0	.....	0	Two fed twice.
Total	45	10		4	

It will be noticed at a glance that there is a very great difference in the results of feeding organs of hogs dead of hog cholera and those dead of *rouget*, but placing the figures side by side will make this difference more perceptible, as is seen below:

TABLE SHOWING DIFFERENCE IN RESULTS OBTAINED BY FEEDING ORGANS OF HOGS DEAD OF HOG CHOLERA AND ROUGET.

	Rouget.	*Hog Cholera.*
Total number fed	45	37
Number dead from feeding	10	34
Percentage dead from feeding	22	92
Sick and recovered	4	0
Shortest period between feeding and death	3	6
Longest period between feeding and death	6	32
Average period between feeding and death	4	15

This table brings out a very striking difference in the susceptibility of hogs to these two diseases when the virus is administered in this way. The proportion which died from hog cholera is seen to be more than four times what it was in the *rouget* experiments; the shortest period between feeding and death was only half with *rouget* what it was with hog cholera, and the longest period was with hog cholera more than five times what it was with *rouget*, while the average period between feeding and death was nearly four times as long with hog cholera as with *rouget*. Considering the considerable number of cases with each disease these differences are so remarkable as of themselves to make us suspect that the diseases experimented with in the two cases are not identical.

4th. *Effects of the virus on pigeons and guinea pigs.*—A method long used in experimental medicine to determine the nature and identity of diseases is the inoculation of other species of animals than those which are spontaneously affected. Thus Arloing, Cornevin and Thomas, in their remarkable investigations which demonstrated that the two diseases called charbon fever and symptomatic charbon were not essentially identical, as

had been suppposed, but were entirely different and distinct, based one line of their argument upon the different effects which followed the inoculation of certain species of animals with virus from the two sources. Rabbits were found very susceptible to charbon fever, but very refractory to symptomatic charbon, and bovine animals were equally susceptible to the former disease at all ages, while their susceptibility to the latter was confined to the period between six months and three or four years of age.

Now, if we study the virus of *rouget* and hog cholera from the same point of view we will find even stronger evidence that they are distinct from each other. *Rouget* virus is extremely fatal to pigeons, and one of these birds inoculated with only a fraction of a drop of blood from an affected animal is almost certain to contract the malady and die from it. The experiments of Pasteur, Schutz, Lydtin, and many others demonstrate this fact. On the other hand Klein has made many experiments from which he maintains that pigeons are entirely insusceptible to the virus of pneumo-enteritis of hogs in England. The writer's experiments in this country show that while pigeons are quite refractory to hog cholera virus they will contract the malady if inoculated with a large dose of a virulent culture.

Again, the experiments of Schutz, Loeffler, Cornevin and others show very conclusively that guinea pigs are entirely insusceptible to the virus of *rouget,* and they cannot be made to contract it even when inoculated with enormous doses. On the other hand Klein found these animals partially susceptible to the contagion of pneumo-enteritis. In the experiments of the Bureau of Animal Industry guinea pigs have been found *extremely susceptible* to the virus of hog cholera, and some of these animals have sickened from doses of less than one-two-hundred-and-fiftieth of a drop.

Unless I am greatly mistaken, these experiments are, of themselves, sufficient to show a profound and radical difference in the nature of the virus from these diseases which Dr. Liautard considers identical. I do not believe that in the whole domain of pathology an instance can be cited where the virus of any one

disease, affecting the same species of animals, differs so widely in its characters, even though found in different countries.

In another paper equally clear distinctions of a different nature will be pointed out.

5th. *Difference in symptoms between rouget and hog cholera.* Without going into details we desire to call attention to the fact that the common names of the plagues of different countries indicate the most characteristic difference between these diseases. Thus in France it is called *mal rouge* (red disease), *rouget, erysiple contagieux* (contagious erysipelas), and in Germany we find the common name to be *rothlauf*, which is equivalent to erysipelas. All of these names indicate the most prominent symptom of the disease, viz.: the red and erysipelatous appearance of the skin. On the other hand, the American name, hog cholera, while it may not give an entirely correct idea of the nature of the malady, certainly indicates that its most constant and prominent symptoms are due to an affection of the bowels.

It is true that redness of the skin is sometimes seen in the American disease, but it is not at all constant, and it is only in rare instances that it is sufficiently intense to be called erysipelatous. Disturbance of the bowels is also seen at different times in *rouget*, but, like redness of the skin in hog cholera, it is by no means a constant symptom. In these characters there is a very marked difference in the two diseases.

6th. *Difference in the appearance of the organs after death.* The post-mortem examination justifies the distinction which has been drawn above from the symptoms. One of the most constant and characteristic lesions of hog cholera is ulceration of the large intestines—an ulceration which is almost invariably present except in those cases which succumb within a day or two after the beginning of the attack. This ulceration is so apparent, so extensive, so impossible to overlook that we cannot believe it would escape the eyes of the Continental veterinarians who have studied their diseases so thoroughly. Neither Lafosse, Bénion, Zundel, Loffler or Lydtin mention ulceration of the large intestines. Schottelius found ulceration in one case, and Cornevin states that

in acute *rouget* there is ordinarily no changes in the cæcum; in the colon the changes are not as constant as in the small intestine; and then he quotes Klein's description of the ulcers observed in England. In the chronic form of *rouget* Cornevin makes no mention of these ulcers, although in this form of hog cholera they are almost invariably the most prominent lesion. We consider this to be a radical and essential difference between the two diseases, and we have failed to find any explanation of it which would justify us in considering *rouget* and hog cholera as identical diseases. The pneumo-enteritis of England, which has been so well described by Klein, is much more closely allied to our hog cholera and may be identified with it, but it certainly is very different from *rouget*.

7th. *These diseases caused by different microbes.*—The microbe of *rouget* is now very well known and has been thoroughly studied. It is peculiar in its appearance, in its manner of growth, and in the way in which it affects animals. It is a fine bacillus, resembling the bacillus of the septicæmia of mice; it multiplies in the blood and in the various organs of the body, and is very easily detected when examinations are properly made. We have this microbe in cultivation in our laboratory, obtained from the Pasteur vaccine; have made inoculation experiments with it and demonstrated its presence in the inoculated animals. We have in this way confirmed the work of the European investigators, and have satisfied ourselves that the disease which it produces is very different from that produced by hog cholera virus. This germ we have never found in any case of hog cholera.

The microbe of those cases of hog cholera which we have studied—and they are now numbered by hundreds—is an entirely different organism; it differs radically in shape, method of growth in various substances and in all its characters, from the bacillus of *rouget*. After learning its peculiarities by long study we find it an easy matter to detect it in the organs of diseased swine; it is easily cultivated in the laboratory, and produces very marked and uniform effects when fed or inoculated.

RESUME OF DIFFERENCES BETWEEN HOG CHOLERA AND ROUGET.

Hog Cholera.	*Rouget.*
Period of incubation from five to twenty-one days; average in summer seven days and in winter fourteen days.	Period of incubation from one to six days; average three days.
Average duration of disease eight to ten days.	Average duration of disease two days.
Proportion which die from feeding upon organs of diseased animals, 90 per cent.	Proportion which die from feeding upon organs of diseased animals, 20 per cent.
Average period between such feeding and death fifteen days.	Average period between such feeding and death four days.
Guinea pigs very susceptible, pigeons refractory.	Pigeons very susceptible; guinea pigs entirely insusceptible.
Ulcers of large intestines almost constant; skin symptons exceptional.	Inflammation of skin almost constant; ulcers in large intestines very rare.
Bacillus of *rouget* not present, but instead of it an entirely distinct microbe.	Caused by a fine bacillus, well known and easily demonstrated.

In the above we have attempted to give an unbiased statement of the reasons which have led us to consider *rouget* as an entirely different and distinct disease from our hog cholera. These reasons appear to justify this view, and the differences pointed out are so radical that we are unable to harmonize them. The fact of the plagues being caused by two distinct germs is of itself sufficient to separate the diseases; but if we did not know this or refused to admit it there still remains sufficient differences in the other characters to make it necessary for us to consider them as distinct maladies.

Here, then, is the true reason why Pasteur's vaccine fails to prevent hog cholera—it is the virus of another and an entirely distinct disease, and as an attack of one disease does not protect from another disease, so Pasteur's virus cannot prevent hog cholera.

Since writing the above we have received the latest report of the German Imperial Health Office, and in that we find a report of investigations of *sbhweineseuche* (swine plague) by Dr. Schutz, which furnishes complete and unequivocal confirmation of our views. He has demonstrated that *schweineseuche* and *rothlauf* are entirely distinct diseases, with different symptoms and caused by different species of microbes. We commend that report to the careful consideration of the gentlemen who have felt so certain that we were wrong in making a distinction between *rouget*

and hog cholera, and we tender the gratuitous observation that it would still be premature to jump to the conclusion that even the German *schweineseuche* is identical with our hog cholera.

REPORTS OF CASES.

FRACTURE AND SYNOVITIS OF THE TEMPORO-MAXILLARY JOINT—RECOVERY.

By J. P. Wilson, D.V.S.

Having treated a case of open joint, which resulted favorably, I concluded to report it.

On April 2, 1886, a two-year-old filly was brought to me for examination. The history was as follows: about September 20, 1885, the animal was kicked on the temporo-maxillary articulation, and was put under treatment by an empiric practitioner. The treatment consisted of opening the wound with an ordinary pocket knife, and injecting into it with a very small syringe once daily, a carbolic solution. The above treatment was kept up for six months. The animal was plethoric when first injured, but had become emaciated to a great extent. Mastication was evidently performed with great pain, and the muscles of mastication on the injured side were atrophied. The wound was discharging very little pus, but it had a very offensive odor characteristic of necrosis. From the repeated cutting which had been employed in the former treatment the cicatrisive tissue had formed to the extent of an inch and a half in thickness, so that a diagnosis was difficult until some of it was removed. But by the use of the probe, which was with difficulty introduced, a diagnosis of the fracture was made involving the temporo-maxillary articulation.

On April 3d the animal was cast, and an incision made so as to remove the indurated tissue to the extent of two inches in circumference, when a comminuted fracture of the inferior maxilla was discovered, the whole of the condyle being detached. The detached pieces of bone were removed, and the fractured surface of the inferior maxilla that had become necrosed was scraped, the wound dressed with a dilute solution of hydrochloric

acid and filled with oakum saturated with a solution of carbolic acid. The animal was then allowed to get up, was given some hay, which it masticated with comfort compared to the manner in which it masticated prior to the operation.

April 4th.—Oakum removed and wound washed with carbolic solution; animal eats about two quarts of oats at a feed, and a reasonable amount of hay.

April 5th.—The necrosed portion of inferior maxilla scraped and dressed with a weak solution of hydrochloric acid; appetite about the same as yesterday.

April 6th.—Wound begins to appear healthy; very little odor of necrosis; dressed with carbolic solution; animal given tonic powders in the feed.

April 7th.—Wound doing well; the odor of necrosis gone; laudable pus, and plenty of it; dressed with carbolic solution; appetite improving.

The animal was given tonic powders twice daily in the feed until April 20th. Its appetite became ravenous. The wound was dressed twice daily with carbolic solution until April 25th, when it had to be enlarged externally in order to introduce the syringe, which had a small nozzle. On April 27th injecting was abandoned, and May 2d the patient discharged with wound completely healed. I saw the animal June 10th and it was doing well.

SANITARY LEGISLATION.

QUARANTINE PROCLAMATION BY THE GOVERNOR OF WYOMING.

Gov. F. E. Warren of Wyoming has issued a quarantine proclamation, under date of July 8th, from which we extract as follows:

WHEREAS, A certain contagious or infectious disease, called pleuro-pneumonia, has become epidemic in certain localities, to wit: in the counties of Putnam, Westchester, New York, Kings, Richmond and Queens, in the State of New York; in the counties of Philadelphia, Bucks, Montgomery, Delaware, Chester and Lan-

caster, in the State of Pennsylvania; in the counties of Bergen, Hudson, Morris, Essex, Union, Somerset, Hunterdon, Middlesex, Mercer, Monmouth, Ocean, Burlington, Camden, Gloucester, Passaic and Atlantic, in the State of New Jersey; in the county of Newcastle, in the State of Delaware; in the counties of Cecil, Harford, Baltimore, Howard and Carroll, in the State of Maryland; in the District of Columbia; in the county of Fairfax, in the State of Virginia; in the counties of Callaway, Boone, Cole, Audrian, Montgomery and Osage, in the State of Missouri, and in the county of Travis, in the State of Texas; and

WHEREAS, It is a well-established fact that cattle from the following States, viz.: Florida, Alabama, Mississippi, Louisiana, and from portions of Texas, Indian Territory, Tennessee, Arkansas and South Carolina, have the capacity to infect cattle of the states and territories lying to the northward of them with a fatal disease, known as Texas or splenic fever, especially when cattle from such localities have been brought to the states and territories with such speed as to prevent their losing the power to communicate said disease; and

WHEREAS, Owners of cattle from said infectious districts are in the habit of driving such cattle a portion of the way and then shipping them to Wyoming Territory, to the danger and detriment of Wyoming Territory; * * *

WHEREAS, Neat cattle from said infected districts have been, at divers times since the discovery of the existence of said disease therein, sold and transported to other sections, without any regard paid to said disease, or the exposure of such cattle thereto, very many of which are now held and owned by persons whose intention it is to transport the same into or through this territory, and by reason of such unrestricted traffic, it is impossible to determine, with the certainty the gravity of the situation demands, without the aid of such owners or persons in charge, whether the said cattle thus intended for Wyoming, or for transportation through said territory, came from or through any of said scheduled localities or not, or whether they, or any of them, have been exposed to said disease or not; either in said localities or in others not herein specified. * * * * * * * *

Therefore, By virtue of the premises aforesaid, and pursuant to said statute, I, Francis E. Warren, governor of the territory of Wyoming, do hereby forbid the importation into or transportation through this territory of any neat cattle that have been brought from any place lying east of the 104th degree of west longitude, which is the east boundary of Wyoming Territory, except only on the conditions and under the restrictions following, that is to say:

1. That said cattle shall only be brought by rail, and shall first be examined by the territorial veterinarian or his deputy at Cheyenne, or such place as he may designate, and if found on inspection to be free from any symptoms of any contagious or infectious disease, then, and in that case, it shall be the duty of the owner or person in charge of said cattle to reasonably establish the following facts in relation thereto, viz.:

That said cattle did not come from or through any of said scheduled localities; and that none of said cattle have been exposed to said disease within four months next preceding their shipment.

2. Upon the truth of each of the foregoing being made manifest to said veterinarian, he shall give to said cattle a certificate, reciting the facts thus proven, and also of his examination thereof, together with a careful description of said cattle; whereupon said cattle shall have the freedom of this territory, and not before; and until such certificate shall be given them, they shall be held in said quarantine yards until such time as the veterinarian shall be satisfied by lapse of time of their freedom from disease, such period of detention to be not longer than ninety days next ensuing their arrival, and if after the expiration of such time they shall not show any symptoms of any infectious or contagious disease, they shall then be allowed the freedom of the territory. * *

NEW MEMBERS OF THE PROFESSION.

HARVARD VETERINARY SCHOOL GRADUATES.

At the examination of candidates for graduation at the Veterinary Department of Harvard Medical College, held in June last,

the following gentlemen received their degree of D.V.M., Doctor of Veterinary Medicine:

A. N. Bigelow.....Norwood, Mass.
Daniel Lee.....Boston, Mass.
W. H. Smith.....Hyde Park, Mass.
W. H. Way.....Chelsea, Mass.
K. Winslow.....Boston, Mass.

CORRESPONDENCE.

A VETERINARIAN WANTED.

Dear Doctor.—Having a good practice, and getting along in years, and having practiced my profession about forty years, I wish to secure a helper, with a view of retiring. Want a single man, of good moral character, and a graduate.

I would be glad to correspond with such a one as above.

Yours very respectfully,

P. O. Box 721, GENESEO, ILL. W. J. SMITH.

SOCIETY MEETINGS.

CONNECTICUT VETERINARY MEDICAL ASSOCIATION.

At the regular meeting of the Connecticut Veterinary Medical Association, held in New Haven on May 4th, the following gentlemen were present: Dr. E. C. Ross, (President), Drs. W. J. Sullivan, A. A. Tuttle, Nathan Tibbals, E. A. McLellan and Thos. Bland.

The minutes of the previous meeting were read and approved.

A letter was read from Dr. W. D. Critcherson, in which he denied ever having been a member of the Association.

After due discussion, the Secretary was instructed to forward the Doctor a true copy of all letters received and to state that the Association considered him a member.

The committee appointed at previous meeting to draft desirable alterations in the By-Laws made their report, which was accepted.

It was unanimously agreed that we shall be known as the Connecticut Veterinary Medical Association, and not Society, as previous.

Dr. Julius E. Gardner was unanimously elected to membership.

Dr. J. G. Knox was proposod for membership by Dr. E. A. McLellan.

Dr. A. L. Brown was not present to read his paper on Aryoturia, but had kindly forwarded it to the Secretary to be read at the meeting.

Although the essayist did not advance anything that might be considered new, still, the subject was treated in the minntest detail. After due discussion a vote of thanks was proposed the essayist.

Dr. A. A. Tuttle will read a paper at next meeting, to be held August 3d.

THOS. BLAND, *Secretary.*

NEWS AND SUNDRIES.

SUBSCRIPTION TO PASTEUR INSTITUTE.—The total amount subscribed to date to sustain the Pasteur Institute in France is $113,-719. The Sultan has presented Pasteur with the grand order of Medjidie, and $2,000, and will send a commission to Paris to study his methods of rabies prevention.

A GOOD RESULT.—Sixteen of the wolf-bitten Russians who were treated by Pasteur have reached Smolensk on their way home, and being in perfect health, have telegraphed their gratitude to their preserver.

PASTEUR'S LABORATORY.—The *Evening Post* quotes as follows from the *Pall Mall Gazette:* "A most extraordinary museum has just been opened in the Rue Vauguelin. It is difficult to say whether it should be called a museum, or a factory, or a farm, or a menagerie. It is in fact all four combined, and grouped together for a purpose hitherto untried, and presenting an appearance hitherto unparalleled. These are the new headquarters of M. Pasteur, and here are to be found cow-houses, sheep-folds, fowl-walks, rabbit-hutches, and dog-kennels. They are all, moreover, fully occupied. On one floor is a laboratory, where the vaccine soups and preparations are made up. Above it a museum, where specimens connected with the new cure are exhibited. There are operating-rooms and rooms for post-mortem investigations and dissecting purposes. Two of the kennels are devoted to dogs in various interesting stages of early or advanced rabies. 'Hen cholera' is communicated, watched, and cured in the fowl-house. The cattle exhibit various stages of vaccination. Human beings have also their provided quarter. A spacious waiting-room is set apart for patients, who troop in daily in picturesque groups—ac-

cording to the French press—representing all nationalities. In the mean time the great savant occupies the former quarters of the Pasteur Institute in the Rue d'Ulm, and devotes himself in dignified seclusion to scientific research."

Discovery of the Microbe of Rabies.—Dr. G. F. Dowdeswell, announces in *The Lancet* that he has discovered the microbe of rabies. He says: "It is micrococcus, not very minute, and of the usual form. It stains, however, with some difficulty; and this accounts for its having hitherto escaped observation. In the cases of dogs which I have as yet examined, its principal seat is evidently the central canal of the spinal cord and medulla oblongata; thence it pervades the other tissues of the central nervous system, occurring (sometimes in vast masses) around the walls of the blood-vessels, and in some cases within the vessels among the red blood-corpuscles. In the cortex of the hemispheres I have found it, but in very small numbers, and, so far, only in the perivascular and peri-cellular lymph-spaces. In the cerebellum I have not found it at all, neither have I as yet succeeded in finding it in the salivary glands. I shall shortly publish the methods by which it may be stained with certainty. I must, however, state that it does not stain by hæmatoxylin, either with or without a mordant, as asserted by Fol. I have repeated his methods carefully. Neither does it occur within the nerve-fibres, as he states; and, lastly, it is fully three times the dimensions which he gives. I may add that it does not occur in the same situation, treated by the same methods, in normal animals. In the one case of a rabid dog, which I had examined to control my previous observations, the tissues were placed in alcohol so shortly after death as to preclude the possibility of the occurrence of septic organisms. In addition to which, all saprophytes, as far as yet observed, stain very readily with the usual aniline dyes, which this microbe does not. I must point out, in justice to the genius of Pasteur, that these observations on the occurrence of the microbe go far to confirm his statement of the seat of the virus; it may further afford a means of diagnosis in any doubtful case." Preparations of the microbe were shown at the meeting of the Royal Microscopical Society on the 9th ult.